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# Four Critiques of the Redistribution Hypothesis

## An Assessment

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## Abstract

This paper discusses and assesses critiques of the author's reformulation of the median voter hypothesis and its testing. The author rephrases and redefines more correctly

the redistribution hypothesis and clarifies its relationship with the median voter hypothesis. He also reviews four types of critiques leveled at his approach.

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This paper—a product of the Poverty and Inequality Team, Development Research Group—is part of a larger effort in the department to study inequality and redistribution. Policy Research Working Papers are also posted on the Web at <http://econ.worldbank.org>. The author may be contacted at [bmilanovic@worldbank.org](mailto:bmilanovic@worldbank.org).

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**Four critiques of the redistribution hypothesis:  
An assessment**

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## Introduction

The empirical testing of the median voter hypothesis broadly defined has undergone a revival. In addition to the papers that do the usual cross-country testing of the median voter hypothesis and often find a rather weak support for it (Dalgaard, Hansen and Larsen 2005, Kenworthy and McCall 2008, Lind 2005, Moene and Wallerstein 2006, Nel 2007, Creedy and Moslehi 2009), a number of other papers redefine, perhaps precisely because of the weak empirical support for the straightforward application of the hypothesis, either the "identity" of the median voter or the domain of the voters. Dhami and el-Nowaihi (2007) redefine the voter so that he or she is concerned with "fairness" of the distribution and not only with individual well-being. Shayo (2009) introduces "individual national identification", which he argues, based on World Values Survey data, to be stronger among the poor voters, and which reduces their propensity to vote for redistribution. To paraphrase Marx, "national identification" here acts as "the opiate of the people." In Hodler (2008), voters, in addition to income, care about leisure; societies with greater preference for leisure also select more redistributive policies. Berenboim and Karabarbounis (2008) propose a "one dollar, one vote" hypothesis whereby the economically stronger the group (poor, middle class, or rich), the more able it is to impose its redistribution preferences. Here, income matters but, up to a certain point, in a different way from the original hypothesis. While in the median-voter hypothesis it is the numbers of the poor vs. rich that matter, here it is their aggregate economic power. Tanninen and Tuomala (2001) and Scervini (2009) focus on a more exact definition of who the median voter is, arguing that he or she needs to be placed within the "inherent" or market income distribution (not as is more commonly done within the disposable income distribution). Mahler (2006), Chong and Olivera (2008), and Arawatari (2009) redefine the population of voters by endogenizing electoral turnout, that is by showing that inequality itself may influence who votes (e.g., electoral turnout of the poor), which would then, in turn, influence the extent of redistribution that a society selects. Campante and Do (2008) find that greater population density, through the implicit threat that many poor people in close proximity pose to rulers, is conducive to greater redistribution. Finally, there are papers that look at the theory alone, and review the reasons why the

process we observe seems to produce different outcomes from that which a simple model posits (Harns and Zink, 2003; Tridimas and Winer, 2005).

The pervasive use of micro data on income, mostly from the Luxembourg Income Study, and the emphasis on market (or "inherent") income distribution, can be traced to the methodology introduced in my paper published as the World Bank Working Paper No. 2246 in December 1999 ("Do more unequal countries redistribute more? Does the median voter hypothesis hold?"), later published in the *European Journal of Political Economy* (Milanovic, 2000) and which for the first time used household level data derived from household income surveys to test the hypothesis, and in the process reformulated the hypothesis itself. I proposed that the correct data to test the hypothesis had not been previously used. I tested the median-voter hypothesis by calculating the gain realized by different deciles of income distribution when people are ranked by their pre-redistribution (market) income. This was in the spirit of the hypothesis as originally formulated by Meltzer and Richard (1981): based on their *pre-redistribution* income, people vote for the level of taxes and type of government expenditures. Previously, however, the test was done in such a way that people appeared to vote on tax-and-transfer combinations based on their ranks in *post-distribution* income or the distribution of disposable income, which of course is logically wrong, since disposable income is the outcome of the redistributive process.<sup>1</sup> The reason for this approach, so obviously wrong at the slightest reflection, was probably because the authors were unfamiliar with household surveys and may not have realized that a given (say, bottom) pre- and post-redistribution income deciles may be composed of very different people. Moreover, the authors might not have been aware that micro data on pre-redistribution income deciles, or more generally fractiles, even if not widely available, do exist.

While the approach that I used is obvious, the interpretation of the sharegain (the difference in the share of a given decile in post- and pre-redistribution income) to directly measure redistribution has been questioned. Four types of criticisms have been leveled at

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<sup>1</sup> Market income *minus* direct taxes *plus* government cash transfers equals disposable income.

my approach and interpretation of the results. My objective here is to set out the critiques, discuss them, and assess their validity.

By way of introduction, I should clearly explain how my analysis was done. For each country and year (using household-level data from surveys available through the Luxembourg Income Study, LIS), individuals were ranked by their (household per capita) market income, from the poorest to the richest, and grouped in deciles. Market income is the sum of wages, property incomes, self-employment income and imputed own consumption. In addition, I defined *marketP* income (called *factor P income* in my 2000 paper), which is equal to market income *plus* state-funded pensions. The reason why state-funded pensions are included as part of market income is that they can be regarded as deferred wage payments through a contractual obligation incurred by the state to its wage-tax payers. Market (or marketP) income can also be called pre-redistribution income because it is the income received before the government intervenes through its direct-tax-and-transfer policy.<sup>2</sup>

To assess redistribution directly, I then observed how the share of each market income decile (that is, the *same people*) changes in the move from market to gross to disposable income. For simplicity, we can skip gross income, which is equal to market income plus government transfers. Consider the share of the people in the poorest decile (according to market income) in market and disposable income. If a tax-and-transfer system is in their favor, they have a larger share of disposable than market income. The difference between the two shares is called sharegain. The empirical results showed that the sharegain was larger for the (market income) poor deciles and then declined before becoming negative for the rich. In other words, the beneficiaries of the redistribution process were the market-income poor. The results also showed that as the market income share of *any* decile goes down, its sharegain becomes larger. For the rich, this means that if market income distribution becomes more equal, their sharegain will be less negative.

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<sup>2</sup> Government cash transfers include unemployment benefits, child benefits, social assistance, and alimony payments.

For the poor, it means that greater redistribution (greater sharegain of the poor) will tend to compensate for higher market income inequality.

The last point is of crucial importance. It implies that a more market unequal state of affairs in a country will tend to lead to a more generous redistribution in favor to the poor. It is here that it becomes important to distinguish between (what may be called) the *redistribution hypothesis* and the *median voter hypothesis*. The latter can be, as is conventional, defined by saying that the median voter is a decisive voter, and hence that his or her position vis-à-vis the rest of the distribution (e.g., vis-à-vis the mean or the top of income distribution) will determine the voting outcome in such a way that a lower relative position of the median voter will result in greater redistribution. The advantage of the median voter hypothesis is that it is firmly based in theory. The disadvantage is that it cannot be tested with sufficient precision. Even the way that Meltzer and Richard (1983) originally tested it using the median-to-mean gross *earnings* ratio was unfortunate for at least two reasons: (a) voters are a much broader category than wage-earners, (b) people vote depending on how they believe their family income will be affected. Regarding point (a), is it reasonable to assume that a person with a very high capital income and no wages would vote for high taxes? Regarding point (b), is it reasonable to assume that two spouses, one with high and another with low earnings, would vote differently from one other and not as a couple? Should we expect that families composed of a high-earning spouse and a non-working spouse would split their vote? It is clearly not reasonable – in either case.

The second problem encountered in empirical specifications of the median voter hypothesis is even more serious: (c) who exactly is the median voter, and (d) against whose income does the median voter compare own income? As for (c), Bassett, Burkett and Putterman (1999), for example, redefine the median to be a “pivotal” voter, that is, the one who is median among the voting population, and since the poor tend to vote less, the pivotal voter is to the right of the median (by income). This is similar to the approach that endogenizes voter turnout discussed above. On (d), the common practice, begun by Meltzer and Richard (1983), was to use the median-to-mean ratio (see also Perotti, 1996).

However, Moene and Wallerstein (2001, 2003) have used the 90-10 ratio (income at the 90<sup>th</sup> percentile vs. income at the 10<sup>th</sup> percentile), and Iversen and Soskice (2006) have used the 90-to-median ratio. The exact measure chosen has varied from author to author, seemingly depending on author's preferences, availability of the data (OECD provides the 90-10, 90-50 and 50-10 gross wage ratios), or perhaps contingent on the formulation that yielded the desired result. Barenboim and Karabarbounis (2008) used three measures: gross earnings at the 90<sup>th</sup> percentile, 10<sup>th</sup> percentile and at the median, each normalized by the mean. It is hard to see which one of these numerous ways to define the median voter and to compare his or her income with income of somebody else makes more or less sense. This has led to a fundamental problem of an insufficiently empirically circumscribed hypothesis, and to "groping" for that particular position in income distribution that the "decisive" voter might occupy as well as with whom exactly he or she might compare own income.

## **2. The redistribution hypothesis defined**

There is a way, which I used in my 2000 paper to reformulate the hypothesis in order to avoid the arbitrariness implicit in the search for the elusive "median" or "decisive" voter. My 2000 paper formulated two hypotheses that I propose to call jointly the "redistribution hypothesis":

- (i) More market-income unequal situations are associated with greater redistribution, and,
- (ii) An increase in the market income share of a given decile is associated with a lower sharegain for that decile.

The first part of the hypothesis is easily and clearly tested by looking at, on the one hand, an inequality index (say, Gini) of market income and, on the other hand, the decrease in that index as we move from market to disposable income. We expect to find a positive relationship: the higher the initial (market) Gini, the greater its subsequent reduction, that is.



$$G_{mk} - G_{dk} = \alpha_0 + \alpha_1 G_{mk} + \sum_k \beta_k \Psi_k + \varepsilon_k \quad (1)$$

where  $G_{mk}$  and  $G_{dk}$  are Ginis for market ( $m$ ) and disposable ( $d$ ) income inequality in country  $k$ , and  $\Psi$  indicates country-level control variables including country dummies, and  $\varepsilon_k$  = normally-distributed error term. We expect  $\alpha_1 > 0$  (note that the dependent variable is defined as *reduction*).

The second part of the hypothesis is tested by regressing the sharegain against market income share for *each* decile, that is.

$$S_{d,ik} - S_{m,ik} = \beta_0 + \beta_i S_{m,ik} + \sum_k \beta_k \Psi_k + u_{ik} \quad (2)$$

where  $S_{m,ik}$  is the share of  $i$ -th decile in market (pre-redistribution) income in country  $k$ ,  $S_{d,ik}$  is the share of the *same people* in disposable income, and  $u_{ik}$  is the error term.<sup>3</sup> The difference  $S_{di} - S_{mi}$  is the sharegain. The key expected result is that the coefficient  $\beta_i$  should be negative for all deciles, that is for all  $i \in (1,10)$ . It means that if a *given* decile is better off pre-redistribution, it will gain less through redistribution.<sup>4</sup> Of course, we may be particularly interested in what happens to the poorest deciles, but the negative relationship ought to hold for all deciles.

Both (1) and (2) are readily testable and are broader hypotheses than the median voter hypothesis. (1) and (2) jointly ask whether greater redistribution is likely to happen in more market-unequal environments, but they do not address the exact mechanism

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<sup>3</sup> Note that the people who are in the poorest decile according to pre-redistribution income need not be, and generally will not be, in the poorest decile according to disposable (post-redistribution) income. In effect, many of them are likely to climb much higher. However, we are not interested in comparing the poorest decile according to pre-redistribution income to the poorest decile according to post-redistribution income because this is not the object of the hypothesis. We are interested in finding out how much, on average, the people in the poorest pre-redistribution decile gain through government redistribution process.

<sup>4</sup> My 2000 paper also showed that the coefficient  $\beta_i$  for the bottom decile and quintile is not significantly different from unity. This can be interpreted as indicating that if market income distribution “moves” against the poorest decile, the redistributive system will exactly compensate for that shortfall. Scervini (2010) has argued that with the more recent LIS data this result no longer holds and that rich countries have become less redistributive.

whereby this happens. The median voter hypothesis may provide one such mechanism. However, decisively establishing that it is *the* mechanism crucially depends on our ability to credibly decide who is the median, or rather the decisive, voter. I propose to term this broader hypothesis, the *redistribution hypothesis* because we need to distinguish the hypothesis proper it from one specific mechanism (the median voter) whereby it may be implemented.

We test the two parts of the redistribution hypothesis using the most recent data from Luxembourg Income Study for 20 OECD countries covering the period 1967-2005 (total number of country/years is 110). We deal here with an (unbalanced) panel, which, especially in terms of econometric issues raised, differs from either static- or time-series analysis as conducted by Meltzer and Richard (1983).<sup>5</sup> Table 1 shows the results of a very simple testing of the first part of the redistribution hypothesis. Column (1) shows the simplest possible test where the only control is unobservable country fixed effect. Column (2) introduces two additional country controls. Obviously, further controls could be brought in but my contention is that  $\alpha_I$  would remain positive and statistically significant.<sup>6</sup> Indeed, we see that its value is stable: a one point increase in marketP Gini is associated with between 0.438 and 0.473 Gini points *greater* reduction brought about by the process of redistribution. In other words, a more market-unequal state of affairs is associated with greater reduction in inequality as government taxes and transfers cash incomes.

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<sup>5</sup> I am grateful for this point to an anonymous referee.

<sup>6</sup> One obvious additional control is democracy but in this sample of established democracies its between-country variability is very small, and in most cases, for a single country it is also time-invariant and thus cannot be included in a country fixed effect regression.

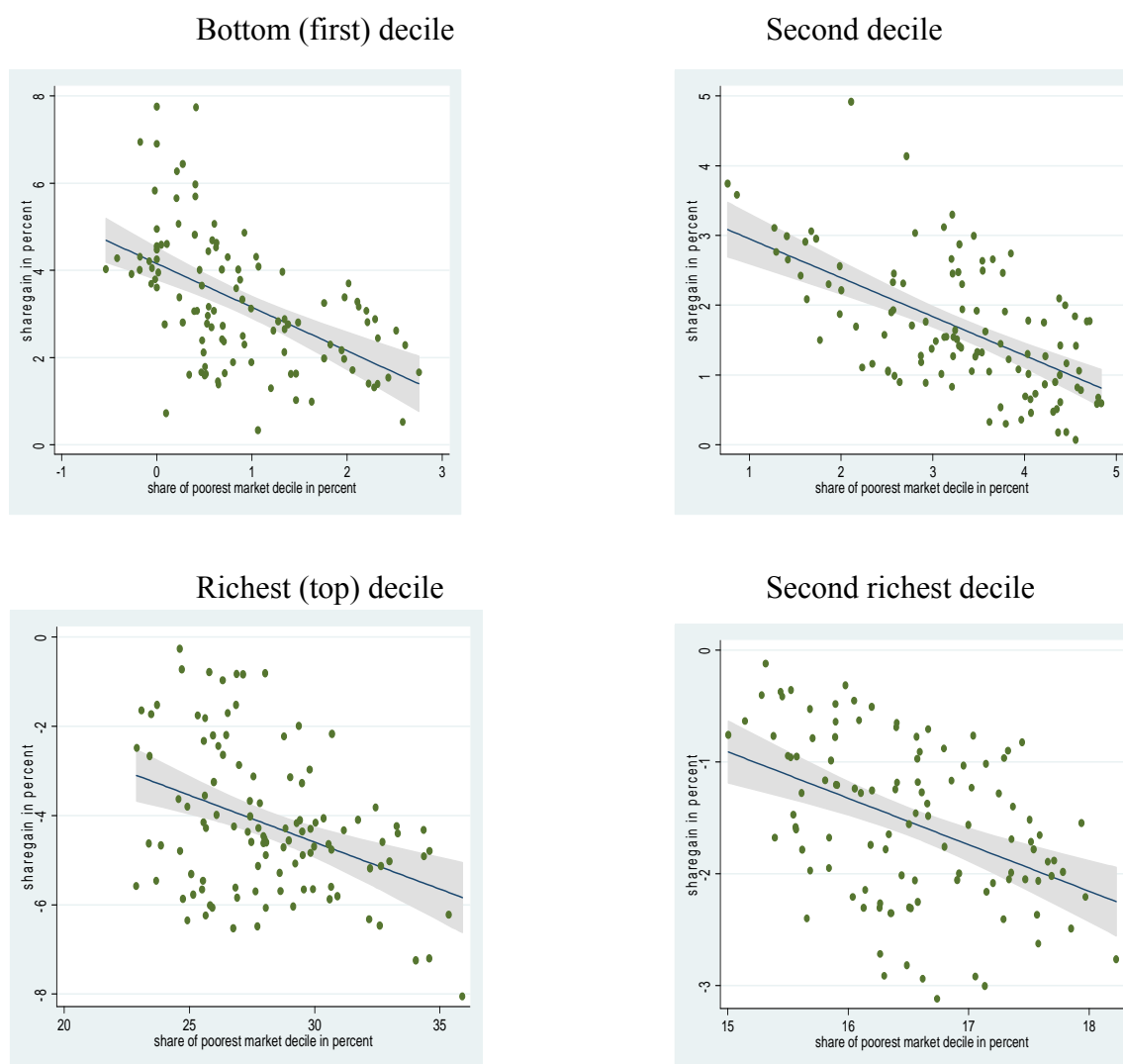
Table 1. Simple regressions of relation (1)  
Dependent variable: Gini reduction through the redistribution process  
(Gini of marketP income *minus* Gini of disposable income)

	(1)	(2)
Gini of marketP income	+0.438** (8.9)	+0.473** (8.2)
Openness		+0.002 (0.9)
GDP per capita (in logs)		-0.004 (-0.6)
Constant	-0.010** (-5.3)	-0.070 (0.3)
R <sup>2</sup> (within)	0.47	0.54
F value	78.7	30.7
Number of observations	110	100

Note: Regressions are run as country fixed effects. Openness is defined as (exports+imports) over GDP. .  
t-values between parentheses. \*\* = significant at less than 1% level.

Let us move to the second part of the redistribution hypothesis. Using the data from the same source, figure 1 shows a simple two-way relationship between the sharegain and marketP income share for the two bottom and the two top deciles. For the bottom decile, the regression coefficient  $\beta_1$  is -0.91 (with  $t$ -value of over 6). The interpretation is that if marketP income share of the bottom decile increases by one percentage point, its sharegain will be reduced by 0.91 percentage points. For the second poorest decile,  $\beta_2$  is -0.78 (also highly significant). For the top decile,  $\beta_{10}$  coefficient amounts to -0.29 ( $t$ -value of over 7) and for the second highest (ninth) decile,  $\beta_9$  is -0.69 ( $t=9.4$ ). The situation is analogous with the other deciles. Thus, equation (2) performs as expected.

Figure 1. The relationship between sharegain and initial marketP income share



Note: Decile share in marketP income on the horizontal axis. Sharegain (on the vertical axis) is the difference in a given marketP decile share in disposable and marketP income (expressed in percentage points). 95 percent confidence interval shaded.

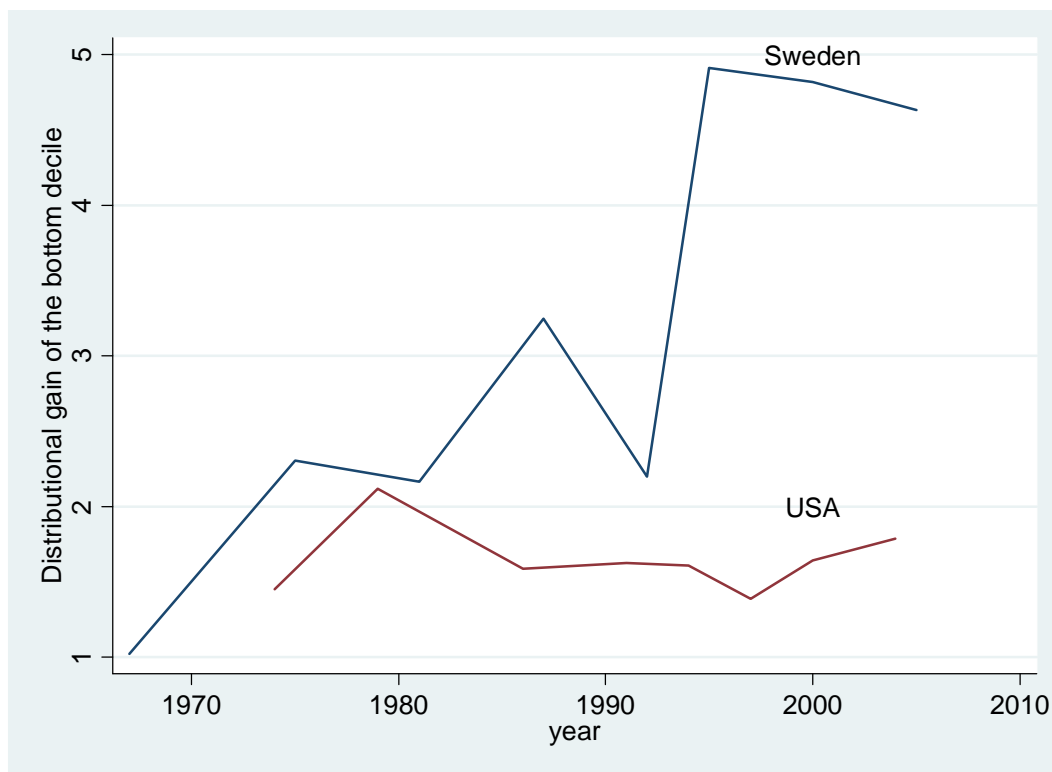
After this sketch of my 2000 approach, and its short update, I now move to the critiques. They fall into four categories.

### **3. The four critiques**

#### *3.1 Country effects*

The first critique is the easiest to deal with because it is based on a misunderstanding of the methodology used in my paper. The regressions were run with country fixed effects (not as pooled cross-sections), and the identification therefore does not come from inter-country differences. In other words, the results do not allow us to conclude, for example, what accounts for greater redistribution in Sweden than in the United States, or in European countries in general than in the United States (see Alesina and Angeletos, 2005). They allow us simply to state that if market income inequality in either Sweden or the US increases, redistribution will go up and the sharegain of the poorest decile will increase. But the level of the sharegain in both countries may still be vastly different. And indeed, if we look at Sweden and the US, it is (see Figure 2).

Figure 2. Sharegain of the bottom decile in Sweden and the United States (1967-2005)



Note: Sharegain defined as the difference in the share of the bottom decile (based on marketP income) in disposable and marketP income.

Hence the critique, as for example in Dhami and el-Nowaihi (2007, p.9), that because (i) both Sweden and the US have about the same market inequality,<sup>7</sup> while (ii) the extent of redistribution is different, the model must be faulty, is based on a misunderstanding of the way the empirical analysis was conducted. The *level* of redistribution in both Sweden and the United States (or any other country) may be explained by other “deep” variables like history of social policy, culture, ethnic homogeneity, trust, prospects of upward mobility, aggregation of political preferences (organization of the political system) etc. The model accepts all of this, and accounts for the unobservable effects through its use of the country dummies; its argument is simple:

<sup>7</sup> This is not exactly true but the difference is small. For example, in 2005 or 2004, Gini for US market income is 50, for Sweden 46. The difference is larger in the case of *marketP* income.

greater market inequality in any county will (controlled for all other observable and unobservable factors) lead to greater redistribution.

### *3.2 Incentives or endogeneity*

The second type of critique is more sophisticated. In several seminar presentations, as well as in some writing (see Berenboim and Karabarbounis, 2008, pp. 10, 13), I have been faced with the argument that the measured effect is not “net” in the sense that the more redistributive states of affairs (I use this term deliberately to avoid comparing countries) will show an “unwarrantedly” high measured redistribution because they affect people’s incentives. Thus, if unemployment benefits exist and are generous, people will more easily accept to be unemployed, will consequently have very low or zero market incomes, and the sharegain (thanks to the existence of generous unemployment benefits) will be large. Were unemployment benefits less generous, people will not go so easily into the unemployed status, they would not be so market-income poor, and the sharegain would be less. This is not a new problem. Introduction of any government intervention (minimum wage, schooling fee, water user charge) leads to the same incentives issues which have been extensively debated in the literature (for a review of US evidence, see Moffitt 1992, and more recently Moffitt, 2002).

Here, the argument is that, due to endogeneity, we are likely to overestimate sharegain and thus redistribution in more generous welfare regimes. Taken literally, this critique is correct and seemingly irrefutable. However, the critique has two weak points.

First, by focusing on the incentive effects of some transfers (like unemployment benefits), it fails to realize that the same argument can be made for any other social transfer. Sharegain from child benefits is not exogenous either. Depending on the generosity of child benefits, people may have more or fewer children (there is a long history of pro-natalist policies in many countries) and this will also affect the sharegain. Families with many children will be, using pre-redistribution household per capita income to rank them, classified in low deciles. Since they are the recipients of child

benefits, the sharegain for these low deciles will be large. Again, we may conclude that the system is inherently generous while a lot of its measured generosity is driven by the change in people's behavior. Similarly, depending on the education policy followed in the past, the rate of unemployment now may be higher or lower, and the number of people with low or zero market incomes may vary, and again sharegain will vary reflecting nothing else but past education policies. Other features of human life, such as mating may be affected by the welfare system; for example, whether inheritance is heavily taxed, whether women have the same opportunity as men to go to school.

In all these cases, more generous welfare systems appear as "victims of their own success": by pushing people to adopt behaviors that are more rewarding, the redistributive character of the regimes is exacerbated since both market inequality and the measured redistribution are biased upward, compared to a hypothetical state of the world where such benefits do not elicit the change in the behavior of potential recipients.

The critique, while fundamentally correct, to some extent misses the point because the presence of any benefit can be shown (as we have just argued) to lead to behavior changes and thus to create endogeneity. We would therefore need somehow to assess the redistributive character of the system while not allowing for behavioral changes induced by the system. This is of course extremely difficult because the benefits thus covered must not include only unemployment compensations whose impact is rather immediate, but also the benefits whose impact is felt only in the long run, such as child allowances or education spending. Like the welfare system itself, this critique is also a victim of its own success. Taken literally, it would make any analysis of redistributive effect of various welfare regimes virtually impossible.

To address this problem by the common use of instrumental variables does not seem adequate. The reason is that the problem is "produced" by a background variable, which we may call "tax and benefit system set up or generosity" that affects people's incentives, and incentives in turn affect both independent (market inequality,  $G_{mk}$  in equation 1) and dependent (redistribution,  $G_{mk}-G_{dk}$ ) variables. This would seem to argue



for the introduction of "system generosity" as another right-hand side variable in equations such as (1). "System generosity" is not strictly speaking a non-observable variable. But to include it in a regression, we need to measure it. And to measure it, as we have argued and will again see below, is very difficult, and it is particularly difficult since such a variable would have to measure generosity of a number of government interventions--not only unemployment benefits, but also education, policy toward children allowances, even minimum wage and practically all other government interventions. "System generosity" (assuming that we somehow measure it, or perhaps create several such variables for different government transfers) will strongly covary with market inequality: more generous systems will stimulate behavior that results in greater market inequality. This will both bias *t*-values down and, more importantly, make difficult the interpretation of the results, that is, telling apart the effect of market inequality from that of "system generosity". When "generosity" of the system is zero (government neither taxes its citizens, nor pays any benefits), we cannot retrieve the "natural" relation between market inequality and redistribution, because redistribution in question here is *government* redistribution. With "generosity" of zero, redistribution must, by definition, be zero too. All of these points illustrate the difficulty of an "econometric" solution to the problem.

The second argument against this critique is based on the fact that the existing welfare systems do not emerge spontaneously or randomly. They are brought about as a result of political developments within a nation, and hence if at some point in time a sufficient majority votes in favor of an extension of unemployment benefits or in favor of high child allowances, it must have been willed by that majority. Thus the very fact that the benefits exist in one state of the world and not in another tells us that there is a political constituency in their favor. When voting on them, the voters have to take into account that benefits' very existence may lead to the change in people's behavior and must therefore vote for both (i) benefits' formal eligibility rules and (ii) the change in behavior that they will entail. When we then observe a given extent of redistribution, we can argue that this is the extent of redistribution that the voters wished to enact. In other words, high generosity of the system is not an unplanned event but precisely what the

electoral constituency (or the median voter, if that's the right mechanism) wanted to bring about. Measured redistribution therefore yields insights into peoples' preferences.

### *3.3 Automatic stabilizers*

The third critique is in some ways a follow-up of the second. Here the argument is that the observed increase in redistribution (an increase in the sharegain of the poorest) will tend to be interpreted as an indication that the welfare system has become more generous whereas, in reality, the economy may be simply going through a recession that raises the number of the unemployed, pushes many families into the lowest deciles (measured by market income), and results in an increase in redistribution (see Kenworthy and McCall, 2008, p. 41). In other words, the generosity of the welfare system is still the same but the number of claimants becomes larger as does the observed sharegain.

This is a valid critique and probably the one most difficult to disregard. However we can address it by being careful in our interpretation of the results. If we observe that the sharegain has increased and find no obvious change in social legislation, then we have to look at whether this may be due to the business cycle effects. We should introduce controls for the business cycle (e.g. rate of unemployment) in the regressions such as (1) and (2).

Kenworthy and McCall (2008) also argue that the public, in making its decisions on which tax-and-transfer system it prefers, focuses on legal or "intended" generosity of the system. To find out whether the public prefers a more generous system one needs to focus—they write—on its legal rules, not on the observed amount of redistribution. This is why, in their analysis of eight advanced economies, they use scores of "intended generosity" (developed by Scruggs, 2004) of the pension system, unemployment benefits, child benefits and social assistance.

However, to give preference to formal transfer rules rather than to measured redistribution is not quite such an obvious choice. (This, in addition to the problem, acknowledged by Kenworthy and McCall, that there are difficulties of assigning subjective “generosity” scores to translate legal rules into values that can be used in an empirical investigation.) First, the very fact that the system is more redistributive (even if its legal basis has not become more generous) is important for policy-makers as it is for the public. The fact will certainly attract people’s attention, and the views about desirability or not of such a system will be based on its *actual* redistributive properties—in the last analysis, on the observation of how much the system costs and who benefits from it. Second, as in the previous endogeneity critique, it is important to realize that the political coalition that brought about such a system of redistribution must have taken into account that, in cases of business cycles, it might lead to large redistributions. This, in turn, means that there was a sufficient political consensus to enact such (potentially) generous redistributive schemes, which is precisely the point about which the redistribution or the median voter hypothesis is concerned. For both reasons, therefore, when we assess a welfare system, we may prefer to look at actual redistribution rather than at formal legal rules only.

### *3.4 Mechanical correlation*

The last critique holds that the results are “mechanical”. This is because very market-equal regimes, regardless of the tax rate they choose, will not be able to effect much of a redistribution simply because their starting point is already very egalitarian (see Lind, 2005). To see the gist of this critique, assume that the tax rate is randomly chosen, that is, does not depend on market income inequality. Lind aims to show that despite this assumption, which would seem to invalidate the redistribution hypothesis, we shall be likely to accept the hypothesis that more market unequal states of the world display greater measured redistribution. To see this, imagine a situation where market income is almost equally distributed across the deciles (there may be just infinitesimal differences between the deciles’ average incomes to allow us to rank them). Whether the tax rate chosen by the voters is very high or very low, does not really matter for

redistribution since everybody will pay more or less the same in taxes, and—an unstated assumption by Lind—receive the same amount in the form of transfers. The welfare system would have simply churned money around and the market poor will end up with about the same share of disposable income as they started, rendering the sharegain close to nil. Thus, “mechanically”, market-income equal states of the world will always tend to register low redistribution regardless of the tax rate chosen. As for more market-*unequal* states of the world, their tax rate is also randomly taken to be either high or low, but measured redistribution will always be positive. When we put these two states of the world (equal and unequal) together in an empirical analysis, we shall detect a positive relationship between pre-redistribution inequality and redistribution. Egalitarian market-income states of the world will always have zero or close to zero redistribution, unequal states of the world, on average, positive redistribution. Thus although the tax rate is by assumption randomly distributed, we shall be nevertheless led—mistakenly, Lind argues—to accept the redistribution hypothesis.

Lind’s critique is wrong because he identifies redistribution with the tax rate only.<sup>8</sup> When we investigate the redistribution hypothesis, we are interested in the overall effect of both tax and transfer policies, that is in actual redistribution, and not merely in one side of that equation, taxes only.<sup>9</sup> To see this, suppose that, regardless of pre-redistribution inequality, the welfare system is so finely calibrated that each income class pays in taxes exactly what it receives in social transfers. Redistribution is zero and such a system is, *regardless of the level of tax rate*, non-redistributive. Lind believes that because the link between high tax rate and redistribution does not necessarily hold, my approach must be flawed. But to be redistributive, it is not sufficient that a regime have a high tax rate; it must also transfer income to the poor. To go back to his example of market-equal states of the world that are invariably shown to be non-redistributive, this need not be true if the transfers are directed to the poor. Thus market-equal states of the world may be either redistributive or not, once we take into account the effect of *both* taxes and transfers. Whether they are one or the other is not merely a data artifact.

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<sup>8</sup> Borge and Rattso (2004) also explicitly, and in my opinion, wrongly, do so.

<sup>9</sup> This is what Lind does when in Figure 3 he generates a “mechanical” relationship between redistribution and market income inequality.

In conclusion, when we observe low sharegain for low market-unequal countries, it is not a “mechanical” relation, but a very real one. It shows that in such cases, even the poor, gain, at the end of the game, very little from the tax-and-transfer system.

#### **4. Conclusions**

I have rephrased and redefined more correctly the redistribution hypothesis contained in my 2000 article and have clarified its relationship with the median voter hypothesis. I have also reviewed four types of critiques leveled at my approach. The first is based on a misunderstanding of the regressions run in the paper. The fourth is based on a wrong identification of redistribution with the tax rate alone. The second and third critiques are more valid. The endogeneity critique is fundamentally correct but its consistent application would make practically impossible any judgment or measurement of the redistributive impact of different welfare systems. This is because the upward bias to the measured impact, which is caused by people’s change in behavior, is often difficult or entirely impossible to quantify, particularly so in the case of long-run effects such as those of family formation and education. An econometric solution to the problem is unlikely because of the difficulty of measuring a variable such as “system generosity” and disentangling its effect from that of market inequality. The third critique (business cycle) is probably the strongest. It highlights the fact that even if (i) the underlying structure of the welfare system is unchanged, and (ii) the behavioral parameters of people unchanged as well, yet there could be a measured change in the sharegain due to the changed external environment (e.g., increased rate of unemployment). This imposes on researchers a duty, when they compare different states of the world, to look at business cycle variables (and to control for them, if they can). It does not invalidate the methodology. It just enjoins us to be more careful when we make our conclusions.

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